Remarks on Simple Relativity and the Relative Velocity of Light.[1]

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II.

The Relative Velocity of Light.

CONSIDER once more the assumption that is either tacitly or confessedly introduced into the establishment of the Larmor-Lorentz transformation and the consequent composition of velocities.

It is this: that the velocity of light outside and far away from matter is absolute; in the sense that it will be measured as the same velocity by every observer, no matter what his relation may be to space and time—*i.e.* no matter where or when he exists, or at what unknown speed he may be moving, not even if his speed were infinite.

A very extraordinary idea that, and one difficult to believe. It is true that it follows from the equations previously written down by Larmor and Lorentz, but they were originally limited to the small range of u/c that covered all practicable observations, and so were not meant to be of universal application and pressed into infallible consequences. The merit, or demerit, of Einstein is that he had no such compunction, and was ready to follow the argument whithersoever it led; and the result—made possible by his wonderful grasp of recondite machinery which he annexed from pure mathematicians, especially recondite when gravitation was included—was a far-reaching effort towards a universal synthesis, in the course of which a few definite features amenable to observation emerged—with the known brilliant results.

Now that the velocity of light in free æther is constant is admitted by everybody, the only reasonable alternative would be some dependence on wave-length, which would mean that the æther was coarse-grained; and that is experimentally negatived by several phenomena and by all manner of determinations of what used to be called a ratio of units, "v," but is more intelligibly and satisfactorily called a measure of the product of the magnetic and electric ætherial constants μK.

But that the relative velocity of light, determined by an observer travelling with speed u to meet it, should still appear the same, and be independent of his motion, is curious, not to say paradoxical. The relative velocity of the observer and the light must be $c+u$—common-sense forbids otherwise,—but if he seeks to measure it he will get, we are told and inclined to believe, not $c+u$, but $(c+u)\div(1+cu/c^2)$, and that is simply c.

So far as I know, no one has ever measured the apparent velocity of light from a star or from one of those spiral nebulæ from which the earth is receding at hundreds, or even thousands, of miles per second. It is not easy to see how it can be done, for the readily observed Doppler effect is always attributed to relative motion of source and observer; and if those are relatively fixed it has been definitely shown that no steady motion of the medium has any observable influence on either direction or frequency (Phil. Trans., 1893, vol. clxxxiv., p. 784). Gusts, however, cause wailing; and by utilisation of the variation of an already occurring Doppler effect something may be done (*l.c.*, p. 785). But, in view of the universality of the above transformation equations, we may admit that it is unlikely that any result other than c will be obtained. It is by assuming the velocity of light constant that the recession velocity is measured; the whole observed retardation is naturally attributed to relative velocity of source and observer; though if we could be sure that all the observed relative velocity really belonged to the receiver, and none of it to the source, we should know that the reason we were able to observe an apparent change in frequency was because of the resultant speed at which we received the waves. But that is just the difficulty—we cannot tell how much of the recession belongs to the source and how much to the receiver. If we could know the observer's speed through the æther we could clearly say that he met the waves more slowly or more quickly than he would otherwise get them; and this reasonable statement has never been disproved by observation.

We ought not to claim, therefore, as some philosophers do, that the fundamental hypothesis of Einstein about observed velocity of light has been directly verified and is a sound basis on which to found a theory. The hypothesis does not justify any theory, though a successful theory may justify the hypothesis. A mistaken claim for what has been done by experiment is often made; and as clear statements are always valuable, whether right or wrong, I select for quotation one from Lord Haldane's recent book, "The Reign of Relativity," on p. 82:—

> Long before 1905 it had been found by experiment that the velocity of light appeared to be always 186,330 miles per second, whether the passage of its rays was towards us while we were at rest with regard to its source, or whether we were ourselves moving towards that source.

Now whether what is here asserted to have been "found by experiment" about the velocity of light be a fact or not, no observation of a discriminating kind had been made before 1905; and I would myself deny that any such observation has been made since. Certainly no experiment of the direct kind suggested in this quotation has ever been made—it is doubtful if it can be made. Every purely terrestrial measurement of the velocity of light has been made, and must necessarily be made, on light which has travelled round a contour, or, what is the same thing, which has gone and returned over the same path. Such an experiment proves nothing, either for or against a discrepancy due to the observer's motion, in the measured 186,330 miles a second. In a to-and-fro journey there is complete compensation for any possible discrepancy in speed so far as small quantities of the first order, involving the ratio u/c, are concerned. The only outstanding discrepancy to be expected is of the second order of minutiæ; and that, as many of us think, is systematically neutralised by the FitzGerald-Lorentz contraction, which, though it is a consequence of the electrical theory of matter, is stigmatised as an unreal contrivance, a mere invented refuge, by the philosophers above referred to.

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[1] Continued from p. 719.

Relativity has only to do with second-order effects; it essentially depends on ds^2, the square of a small interval; but the statement above quoted is not entirely about second-order effects; it relates to the first order—to a journey in one direction—and would require for direct verification an observation of the difference in the time of a single journey, when the observer is moving (*a*) with, (*b*) against, an æther stream.

The nearest approach to a measurement of this kind that might conceivably be made would be a vastly improved determination of the velocity of light by a method based on the observation of some periodic feature in Jupiter's satellites during the course of Jupiter's year. To make a determination possible at all, the earth must be moving either to or from Jupiter at the time—it does not matter which—and the chance of obtaining a positive result depends on the varying angle which the line joining earth and Jupiter makes with the sun's way, or rather with the direction of locomotion of the solar system through the æther, whatever that direction may be.

But I think it is generally agreed—subject, however, to the opinion of the chief authority on those motions, Prof. R. A. Sampson—that the gravitational theory of the satellites, perturbed as they are by each other as well as by the oblateness of Jupiter, is not yet nearly perfect enough to enable us to decide the question whether the velocity of light deduced from their eclipses is dependent on the season of Jupiter's year,—in other words, whether light appears to reach us with the same speed when we are looking at Jupiter down-stream as it does when we are looking at him up-stream (see Phil. Trans., 1893, vol. clxxxiv., pp. 746, 779, and 785). For we have no means of determining the instant at which the light starts from Jupiter; all that we can really observe is the time that light takes to transit the distance travelled by the earth in the interval between two eclipses.

Apart from all astronomical observation, however, it has been claimed that the rather recent pair of experiments of Prof. Majorana, with moving mirrors (*Phil. Mag.*, February, 1918, p. 163, and January, 1919, p. 145), do establish the thesis that the observed velocity of light is independent of the relative motion of observer; but they, too, are observations made on a to-and-fro journey, and, therefore, for the present purpose, are beside the mark. If light were a projectile, it could be hit forward by a moving mirror, like a cricket ball, but no one can suppose that any kind of impact can alter the subsequent velocity of waves through a medium, nor is it to be supposed that motion of a source can affect the travelling rate of waves which it has emitted and abandoned.

More Careful Discussion of Doppler Effect.

Motion of the source does not affect velocity, but if a moving source emits waves at constant frequency n, the wave-length ought to be different in different directions θ, and this modified wave-length,

$$\lambda' = \frac{c\cos\epsilon + u\cos\theta}{n},$$

can be observed by a fixed observer, and, when compared with the normal $\lambda = c/n$, is called the Doppler effect. The small aberration angle ϵ, between ray and normal to wave-front, is defined by $c\sin\epsilon - u\sin\theta = 0$.

If, however, the source is fixed in the æther, and only an observer is moving, the velocity and the wave-length are both quite normal; but the frequency with which the waves are encountered by the observer will depend on the speed and direction of his own motion. Consequently there is again an observable Doppler effect expressed as

$$n' = \frac{c\cos\epsilon + u\cos\theta}{\lambda},$$

to be compared with $n = c/\lambda$.

Hence if an observer steadily chases a source, keeping a fixed distance between them, the two effects—the real wave-elongation and the apparent frequency increase—neutralise each other *exactly* whatever the direction of joint motion, because $n\lambda' = n'\lambda$. So drift through a medium produces no trace of a Doppler effect.

Nevertheless, the two types of effect—one with source only moving, the other with receiver only moving—are not identical; they are the same when both are moving in the same direction, so as to be relatively at rest, but not the same when they are moving relatively to each other. For, writing u/c as a, and taking the case of relative recession between source and receiver, we get, for the observed frequency ratio,—
if it be the source only which is moving,

$$\frac{n'}{n} = \frac{\lambda}{\lambda'} = \frac{1}{\cos\epsilon + a\cos\theta};$$

while if it be the receiver only which is moving,

$$\frac{n''}{n} = \cos\epsilon - a\cos\theta.$$

Hence

$$n'' \text{ is not equal to } n', \text{ but}$$
$$\begin{aligned}\frac{n''}{n'} &= \cos^2\epsilon - a^2\cos^2\theta\\ &= 1 - a^2\sin^2\theta - a^2\cos^2\theta\\ &= 1 - a^2,\end{aligned}$$

which is the square of the usual FitzGerald contraction. The relativity doctrine, in order to avoid recognising any such difference, would presumably distribute this factor between the two expressions, making them

$$\frac{\lambda'}{\lambda}=\frac{\cos\epsilon+a\cos\theta}{\sqrt{(1-a^2)}},$$

$$\frac{n''}{n}=\frac{\cos\epsilon-a\cos\theta}{\sqrt{(1-a^2)}};$$

so that there shall be equality between $n''\lambda'$ and $n\lambda$, and then it is impossible to tell to which body the motion belongs.

Note that the introduced β factor cannot in this case be attributed to a FitzGerald contraction of the grating—if a grating is used as the measuring instrument,—for the aspect of the grating to the incident light, and therefore to the motion under examination, is normal, not tangential. But the law of reflection is interfered with by the motion, in a way investigated in Phil. Trans. for 1893 (vol. clxxxiv., A, pp. 793–800), and the result is to give a modified deviation which will be interpreted as part of the Doppler effect. The discrepancy is reckoned, on p. 798 *loc. cit.*, for any incidence angle i and any drift angle ϕ, as $a^2\cos^2 i\sin 2(i-\phi)$; so it is a maximum for normal incidence and for a drift direction making 45° with the ray.

This might readily have the average value $\frac{1}{2}a^2$ needed to replace the ordinary β factor, but in so far as it yields a factor depending on the angle ϕ its changes *seem* amenable to observation.

In the same Phil. Trans. paper I show (p. 787) that the Doppler effect observed by a moving grating is really an aberration effect, due to the motion being partly across the diffracted rays, although the incident ray may be along the drift. For a grating *must* deviate in accordance with wave-length, whether it be moving or stationary, so far as first order is concerned.

But the question arises, What happens when the grating is drifting partly in its own plane ($\phi=90°$) and thereby suffering a FitzGerald contraction?

The answer seems to be that the extra aberration due to this drift will just compensate the second-order Doppler effect otherwise to be expected from the ostensibly narrower-ruled grating. There are certain possibilities here, however, which need looking into.

Summary of this Portion.

The Einstein formulation seems to justify itself by results, and may be supposed to strengthen the claims of any philosophy suggested by it, as well as to establish the explicit assumptions on which the theory is based; but we should be careful to perceive that justification is of this subsequent inferential order, and that it is not primarily the outcome of experiment—certainly not of any old unexplained measurements. The whole thing depends on the law that we postulate for the composition of velocities. When two velocities in the same direction are compounded, is the resultant velocity $u+v$, or is it really $\dfrac{u+v}{1+\dfrac{uv}{c^2}}$? Einstein's assumption led to the latter as a physical truth, and if that is right it is algebraically undeniable that if one of the component velocities is c, the resultant will be c also; and any such criterion as my old experiment (1892–97) with rotating discs, whereby it was sought to observe a possible difference between $c+v$ and $c-v$, cannot give any positive result. Nor can it, by giving the result zero, prove that v is 0, because, as a matter of simple algebra, if $u=c$, no sort of v can make any difference, not even if it be infinite.

So also Michelson's experiment can show nothing, nor can any velocity compounded with the velocity of light exhibit itself in any way, if that is the true law of composition of velocities in general.

But why into this composition formula should there enter the velocity of light? If, for instance, the composition is between a ship and a tide, or a satellite and a planet, or the usual railway train and embankment, one cannot avoid the question, What on earth has the speed of light to do with it? any more than the speed of sound or of a messenger boy, or whatever agent it is which brings information to an observer. It is true that the law of composition is essential to the principle of relativity, but when we are engaged in establishing that principle it is scarcely fair to assume it.

The curious law of composition is deduced from the Lorentz transformation of space and time to other co-ordinates,

$$x'=\beta(x-ut)\ ;\ t'=\beta\left(t-\frac{ux}{c^2}\right)\ ;\ \beta^2(c^2-u^2)=c^2\ ;$$

and in the establishment of these equations it is assumed that all observers have the same value of c, or that $x^2-c^2t^2$ is invariant.

I apprehend that for this transformation, treated as formal correspondence, there is a good deal to be said, so that any law deduced from it may be true with all its consequences; but it is surely a mistake to say that the measured velocity of light has been experimentally proved constant. So far as the velocity of light is concerned, the reasoning is circular. I suggest that it is also dangerous to adopt a mode of exposition which denies reality to the FitzGerald contraction. Still more is it premature to assume, as more than a temporary conclusion, that no phenomenon demonstrating our motion through the æther of space can ever be discovered; which carries with it the implied suggestion that the inability is because such a medium does not exist; so that not only can all motion be treated undynamically as a purely geometrical or kinematic relation, but so also that in absolute truth there is no difference discernible between a dog wagging its tail and the tail wagging the dog. Kinematically, it is as easy to take the apple as standard of reference as it is to take the earth, but physically and energetically the treatment can only be satisfactory when their combined or reciprocal motion is balanced about their common centre of gravity.

Centres of gravity, however, presumably disappear from relativity; and, what is more serious, so does the conservation of energy. For if there is nothing absolute about speed there can be nothing absolute about kinetic energy. The relativity expression for kinetic energy contains an arbitrary constant; and whether energy is conserved or not becomes a matter of convenience and definition. The claim that relativity pressed to extremes does away with all conservation, as hitherto understood in physics, has been seriously made by the eminent mathematician, Prof. Hilbert, of Göttingen. On the other hand, it might be replied, according to Sir Joseph Larmor, that kinetic energy has always been treated as relative to some other body on which work might conceivably be done, and that the really invariant quantity is not energy, but the integral of energy with time, called "action"; or as it may be regarded, perhaps preferably for some purposes, i times angular momentum.[2] For this appears to be independent of frames of reference—which energy certainly is not.

Acceptance of the theory of relativity correlates results, but does not explain them. The theory does not even seek to explain or account for phenomena: they just are so. It is not a dynamical theory, it is a method of arriving at results, like the second law of thermodynamics and the conservation of energy. The full dynamical explanation remains to be worked out, and it may turn out to be on very much the old lines along which we had previously regarded physical phenomena. The true relation between æther and matter, and how their interaction generates and affects light, is an immense subject, not in the least exhausted, and barely encroached upon, by the perception that certain consequences inevitably follow from an admission that the velocity of light is a critical limiting velocity, which cannot be exceeded, and which when compounded with any other velocity retains its old value.

Whether the properties of the æther can ever be formulated in terms of the same sort of dynamics as we have found so fruitful and effective in dealing with matter is at present an open question. Quite possibly a different dynamics may be needed, one perhaps of which we have as yet no conception; but let us not shut the door on discovery, assume that nothing of the sort can ever be arrived at, and think that pure mathematical abstractions, glorified and complicated sufficiently, can be an ultimate embodiment of physical laws or can adequately express the facts of Nature.

(*To be continued.*)

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[2] Which, by the way, is very suggestive of a constitutional gyrostatic æther structure.

The Conference of the International Union against Tuberculosis.

THOSE who have followed the course of tuberculosis in this country have noted that during the years of the war there was a sudden interruption in the fall of the curves illustrating case-rate and death-rate from that disease. Our work was then in fields abroad. Now, however, that we are getting back to pre-war conditions, peoples and nations are again joining forces in a new campaign against tuberculosis in our civil populations, and at the recent conference in London of the International Union against Tuberculosis delegates from forty nations, including China, Japan, Persia, and Czecho-Slovakia, met to discuss the great question of the cure and prevention of tuberculosis. Science knows no national borders, and it is obvious that the union is anxious to work with men from all nations, and to this end has drawn up a series of tentative regulations in order that when German physicians have composed the differences amongst themselves arrangements may be made for their reception into the councils of the union. The secretary of the old International Association against Tuberculosis appears to have assumed that everything would go on as before, and somewhat injudiciously made an attempt to call the old association together as a rival to the conference of the union of Allies and neutrals held in Paris last year. The wiser amongst his countrymen were against this, and at present the German physicians are divided into two camps. For the present the International Union is content to make good its own footing, go its own way, and lay down its own lines of operations, at the same time leaving the regulations so elastic that as asperities are smoothed down and difficulties removed German workers may come in and take their part in its great work; and it is hoped that steps towards this will have been taken when the meeting is held in Brussels next year, or, at any rate, in Washington two years later. By that time the League of Nations may have got under way, and the international character of the union may have become complete.

At the opening sitting of the London meeting the Foreign Secretary, Lord Curzon, and the Minister of Health, Sir Alfred Mond, blessed in no uncertain terms the ideals and work of the union, and their presence no less than their works may be accepted as of good omen that the Government authorities will, in their anti-waste difficulties, remember that a penny wise Health Ministry may be pound foolish where the public health is concerned, and that the same holds good as regards the Board of Education.

Prof. Calmette, in a most interesting opening address characterised by all the clearness of vision and beauty of expression for which this French savant is noted, outlined a new hypothesis